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Can Vouchers Reduce Elite Capture of Local Development Projects? Experimental Evidence from the Solomon Islands

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Abstract

External financing of local public goods can potentially create 'political resource curses' by reducing citizen oversight, exacerbating elite capture, and producing policy outcomes that are sub-optimal for the general population. This paper experimentally tests a novel mechanism that seeks to mitigate elite capture of local development projects. Control communities are provided with block grants to fund local public goods, while households in treatment communities are provided with vouchers that they may either contribute to a public good or redeem at a discount for a private capital good. We find that the use of vouchers as a mechanism for aid delivery increases community participation in local public decision-making, changes the nature of allocation outcomes, and improves community satisfaction with allocation outcomes.

Keywords: public goods, decentralization, elite capture, private contributions

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1 Introduction

As far back as Montesquieu's "Spirit of the Laws",[1] social scientists have argued that the accountability of political authorities is influenced by the nature of public revenue. Where revenues are derived predominantly from rents instead of direct taxation, citizens are less likely to exert demands on their leaders, which in turn adversely affects the delivery of public goods (Brollo et al., 2013; Herb, 2005). The impact of foreign aid on development outcomes has long considered to be limited by such "political resource curses" (Djankov, Montalvo, and Reynal-Querol, 2008; Rajan and Subramanian, 2007). Partly due to such concerns, modalities that involve local communities in the selection, management, and monitoring of development projects have become increasingly popular among aid agencies in recent decades (Casey, 2018; Fung and Wright, 2003). Studies indicate, however, that the local public good decision-making processes prescribed by these modalities are often captured by elites (Mansuri and Rao, 2013) and that they may thereby only localize the political resource curse. Innovations to reduce the extent of elite capture have, to date, met with limited success (Banerjee et al., 2010; Beath, Christia, and Enikolopov, 2017; Olken, 2007, 2010).

In this paper, we present a novel mechanism to reduce elite capture of local public decision-making by channeling external resources to fund public goods through citizens. The mechanism provides households in villages covered by a development program with vouchers which households may either contribute to a proposed local development project or redeem at a discount for private use. By providing villagers with the collective ability to de-fund a non-accountable local authority and by establishing the private opportunity cost of public expenditure, this voucher-based modality seeks to encourage the formation of a fiscal social contract between elites and villagers. As such, it is envisaged that the modality will increase the incentive for local authorities to propose and/or support publicly beneficial projects and will increase villager participation in project selection and monitoring, thereby resulting in higher quality projects.

To test the effects of vouchers on project selection, we administered a field experiment across 80 villages in the Solomon Islands, a country where local authorities have historically exercised authority over local public decision-making. The field experiment was centered around structured community activities (SCA) (Casey, Glennerster, and Miguel, 2012). In each village, 20 randomly-selected adults were each

[1] See "Book XIII. Of the Relation Which the Levying of Taxes and the Greatness of the Public Revenues Bear to Liberty".

provided with 10 notes, which could be redeemed for either 10 Solomon Island dollars (SBD 10, approximatively USD 1.40) each if contributed to a public fund, or SBD 5 each if retained for private consumption. In the control villages, the maximum fund amount (SBD 2,000, approximately USD 300) was provided as a block grant with no individual contributions required and no possibility for households to retain any portion of the grant for private consumption. In both treatment and control communities, the public fund could be used to purchase items selected by participants from a pre-set menu of materials at a local hardware supplier. Importantly, participants made their decisions anonymously, thereby avoiding the potential for intimidation and/or retribution. Apart from the way in which the funds were distributed, all features of the process were the same across both treatment arms.

The results of the experiment indicate that the use of vouchers substantially alters both the selection process and allocation outcomes, although the effects on project implementation outcomes and general welfare are ambiguous. Compared to control villages, the voucher-based modality increases the duration of discussions about project selection among participants and the average number of times community members speak during such discussions. The voucher treatment changes the types of projects that are selected by the group and, specifically, increases the probability of health-related interventions receiving funding. Villagers also perceive project outcomes to be fairer under the voucher scheme. The treatment effects on participation in the discussion and fairness perceptions are stronger for individuals who had not previously taken part in community decision-making. As expected, however, the voucher scheme reduces the volume of funding available to villages, with treatment villages receiving just 79% of funds available (58% of the total available in public good contributions and 21% in vouchers redeemed for private cash payments). With available data, we are unable to assess whether this reduced flow of funds was associated with reduced welfare. Furthermore, with the limited data available, we do not observe differences between control and treatment communities in the speed with which they obtain materials and implement selected projects.

2 Background

2.1 Sample Villages

The study occurred between June and August 2013 across 80 villages randomly sampled from the population of villages participating in the Solomon Islands Rural Development Program (RDP). Launched in 2008, RDP was implemented by the Solomon Islands' Ministry of Development Planning and Aid Coordination (MDPAC) and was supported by AusAID, IFAD, and the World Bank. A community-driven development (CDD) program, RDP financed investments identified by villagers through a participatory process. Existing local institutions, such as tribal councils and churches, planned and managed RDP activities at the community level and supervised implementation of small works.

The 80 sample villages are small, with an average population of 488 people, and isolated. The average travel time to the respective provincial capitals is 12 hours and it takes an average of two-and-a-half days to reach the capital. The vast majority of villagers (82%) rely on subsistence fishing and horticulture. Most villages do not have access to electricity, running water or sanitation. Four out of every five households use rainwater catchments for drinking water, only have access to solar lamps for lighting, and lack access to improved sanitation. In this context, the financing provided by RDP offers a vital opportunity to upgrade local public facilities and services.

Given the isolation of the sample villages, formal government structures are of limited relevance. Most of the villages (85%) are governed by traditional village chiefs, with elected leaders (8%) and/or church leaders (13%) providing local governance services in a much smaller proportion of communities, and a number of villages having more than one type of village leader. All villages have one or more churches, which also serve as the community hall for meetings. Religion is an important part of daily life, with nearly all villagers claiming a religious affiliation.

As in other cases, our sample was limited by budgetary constraints, and the precision of cross-village treatment effects are thus limited for some outcomes. We discuss the minimum detectable effects implied by our estimates with each of our primary cross-village results.

2.2 Intervention

CDD programs, and decentralization efforts more broadly, must contend with

the challenge that local public good decision-making is often subject to capture by elites. Various studies, for instance, note that participants in local public decision-making are generally wealthier, more educated, hold higher social status, and are more politically connected than non-participants (Mansuri and Rao, 2013; Pradhan, Rao, and Rosemberg, 2010; Arcand and Fafchamps, 2012; Mansuri, 2012). As a result, the outcomes of local public decision-making often align with the preferences of local elites (Fritzen, 2007; Labonne and Chase, 2009; Rao and Ibanez, 2005). While such elite capture is not necessarily detrimental to the general interest if it enables better-informed and benevolent elites to exert more influence, villagers perceive that they are worse off in cases where elites have more influence over project selection (Beath, Christia, and Enikolopov, 2017). Moreover, CDD programs are found to adversely affect the quality of decision-making by local leaders (Beath, Christia, and Enikolopov, 2013), a result attributed to the effect of external financing on the accountability relationships between local leaders and the community (Brick, 2008).[2]

We therefore aimed to develop an intervention that provided participants with the power to incentivize public spending that is in line with their own individual preferences. Prior efforts to encourage greater participation by non-elites in the public good selection and implementation process had met with little success. For example, many CDD programs employ facilitators who guide communities through a needs identification and implementation process (Mansuri and Rao, 2013), although the presence of facilitators appears to shift the project choices toward the preferences of the facilitators themselves (Platteau and Gaspart, 2003). Referenda to select local projects from a menu provided by a CDD program improve citizen satisfaction, but do not change the type of projects selected (Olken, 2010), nor increase the effectiveness of implemented projects (Beath, Christia, and Enikolopov, 2017).[3] Rather than alter the process or the participants of discussions around public good funding, we instead chose to provide participants with fiscal control over this funding, while accounting for common free rider challenges.

In the 80 sample villages, leaders were asked to invite all available adults to a community meeting on a specified date. The meeting was typically held in local

[2]A related experimental literature shows the correlation between leaders preferences and community members behavior, from cooperation (Kosfeld and Rustagi, 2015) to contributions to public goods and private investment (Beekman, Bulte, and Nillesen, 2014; Jack and Recalde, 2015).

[3]This result is consistent with evidence on individual valuation of decision processes, independently from decision outcomes (Guth and Weck-Hannemann, 1997; Fehr, Herz, and Wilkening, 2013; Bartling, Fehr, and Herz, 2014; Owens, Grossman, and Fackler, 2014), and on control aversion among individuals (Falk and Kosfield, 2006).

schools or public buildings, protected from outsiders' intrusions. Attendees of this meeting represent the sampling frame for the participants in the experiment.[4] From this frame, 18 villagers (9 male and 9 female) were selected via a random drawing of names. In addition, the two highest-ranking leaders (one male, one female) were selected from among those present at the meeting. The community meeting was then adjourned, with only the individuals selected to participate asked to remain.

In all villages, selected participants were informed that SBD 2,000 had been allocated to fund the improvement of a local non-religious public facility, such as a school, health center, market, toilet, road, or water system. The fund was provided as credit at a local hardware store and permitted the purchase of materials required for the work, such as paint, roofing iron or cement. Villagers were required to provide labor and complementary materials for the selected project. One participant was selected to record community contributions and the use of allocated funds. Participants were also asked to nominate the person responsible for procuring materials from the hardware store.

Facilitators directed participants to discuss the type of project that they believed would most benefit the community, with the goal of reaching a consensus on which project to fund. In order to ensure the norms of discussion and decision-making adhered to those of the community, no structure was imposed on the form of the discussion or on the method of selection of the project. Facilitators did not intervene in the discussion until an agreement was reached, but rather passively recorded who spoke and for how long. At the conclusion of the discussion, facilitators directed participants to complete a form identifying the type of project and which materials they intended to purchase. Following the completion of the form, participants in all sample villages were paid a small fee for participating in the activity.

[4]It is possible that leaders may have selectively invited villagers to the meeting and/or that villagers may have self-selected based on their needs and/or capacity to exercise voice. Using data concurrently provided by a random sample of 10 households in each village, we find that participants have slightly higher ownership of toilets than non-participants, but exhibit lower levels of access to primary schools and health clinics. There is also no difference between the correlation of project preferences of leaders and participants and the correlation of the project preferences of leaders and non-participants. Overall, there is no evidence to indicate that the participant selection process was substantially affected by leaders and/or by self-selection.

2.3 Description of Treatment

Within each province, sample villages were randomly allocated to either the control or treatment group, which differed in the mode of fund allocation. Any treatment-specific information was revealed to participants after they had been selected. In villages assigned to the control group, facilitators informed participants that a block grant of SBD 2,000 would be allocated to fund the community project. In the treatment group and prior to the discussion, facilitators issued each of the 20 selected participants with 10 paper vouchers and explained that each voucher could either be redeemed for cash or contributed to the fund for the community project. If redeemed, vouchers would be worth SBD 5 each, whereas vouchers contributed to the project would be worth SBD 10 each.[5] Following the discussion and project selection, participants in the treatment group were asked by the facilitator to indicate privately how many of the vouchers they wished to redeem and how many they wished to contribute to the community project.

3 Hypotheses

The provision of vouchers to participants may affect selection processes and outcomes by changing the structure of incentives facing local leaders to build consensus. In treatment villages, leaders seeking to maximize funding for their preferred project must ensure the involvement of all participants in the decision-making process and seek direct assurances from participants that they support the project choice. Otherwise, participants who disagree with the project selection may decide to withhold part or all of the funding. In contrast, during discussions in control villages, leaders face an incentive to minimize participation in order to reduce the probability of dissent over the relative benefits of project options.

The use of vouchers to select projects may also increase the willingness of villagers to participate in discussions about project selection by changing the framing of the selection process. In sample villages, decisions over the use of local public resources—such as project selection—are ordinarily the domain of local elites, with social norms discouraging villagers from challenging the decisions of such elites in a public setting. In this context, the use of vouchers potentially creates a new frame for local public decision-making which provides individual participants (especially non-leaders) with

[5]Thus, if a participant redeemed all vouchers for cash, they would receive SBD 50 (approximately USD 7.50), roughly equal to five percent of the average monthly income.

special authority over selection outcomes and induces them to actively participate in the selection process.

In so far as the use of vouchers to select projects increases active participation by community members, vouchers also should increase the extent to which non-leader participants announce their preferences over the menu of projects. Where individual preferences are heterogeneous and not common knowledge, the discussion facilitates their aggregation and increases the probability of their realization. Furthermore, provided leaders prefer the implementation of any project to no project, the use of vouchers should encourage leaders to accept a selection outcome that they do not prefer in order to ensure that participants do not redeem vouchers and thereby deprive the village of a project.

In summary, by increasing incentives for local leaders to encourage participation and to accept project outcomes that they do not necessarily prefer and by relaxing social norms that may otherwise inhibit participation, vouchers should increase participation by marginalized community members, improve the correspondence between participant preferences and selection outcomes, and improve satisfaction both with the process and the outcomes. The study therefore tests the following hypotheses relating the treatment to various outcomes of interest:[6]

1. Vouchers increase participation in project selection, as measured by the duration and inclusiveness of discussions on project selection;

2. Vouchers increase the correspondence between selected projects and preferences of the median non-elite participant and preferences of marginalized participants who do not ordinarily participate in community decisions;

3. Vouchers improve the fairness of the project selection process as perceived by participants and satisfaction of participants with the selected project.

[6]These hypotheses were documented in a pre-analysis plan completed before the data collection. The pre-analysis plan also included hypotheses relating the treatment to the likelihood of projects being implemented and to the quality of projects. However, the available data does not allow us to test these hypotheses, as only a small number of communities had collected the material and started to work on the projects three months after the intervention. For this reason, we do not test any hypotheses pertaining to project implementation. However, the respective results are discussed briefly in Section 5.

4 Data Sources and Descriptive Statistics

4.1 Data Sources

Data to estimate the effects of the treatment and to explore heterogeneity in treatment effects was collected across four stages in all sample villages:

1. Prior to the discussion, a short questionnaire was administered to all selected participants. Participants, including participating leaders, were asked to provide an ordinal ranking of the top three buildings that they believed should be improved with a hypothetical SBD 2000 grant. They were provided with the following nine options: kindergarten, primary school, health clinic, water system, sanitation, market, road, bridge, wharf, or another non-church community building.[7]

2. During the discussion in all sample villages, the facilitator recorded the number of speaking interventions by each participant per five minutes and the total length of the discussion.

3. In treatment villages, participants' voucher contributions were observed after project selection.

4. Following the discussion, an additional short questionnaire collected information on demographic and socio-economic characteristics, prior experience with community organizations, perceptions of the decision-making process and outcome of the SCA, and satisfaction with the local leadership. The survey also collected information on participants' willingness to share resources with others when nothing is expected in return, a proxy of altruism.[8]

5. Information on community characteristics was collected by facilitators from a sample of key informants, such as village elders and other local leaders.

[7] An additional project type, church buildings, was overwhelmingly cited by respondents in the 'other projects' category, and so was assigned a separate category ex-post for the analysis.

[8] This question is shown to correlate strongly with choices in the dictator game, a behavioral game commonly used to capture altruism (Falk et al., 2016). This question was asked after the discussion in order to avoid priming subjects to act cooperatively, as evidence shows how focusing individual attention on social norms affects behavior in subsequent experimental tasks (Krupka and Weber, 2009). Although this raises the possibility that the altruism measure is influenced by the treatment, we do not find evidence of a treatment effect on it (see Table 1).

6. Approximately three months after the discussion, an enumerator returned to 65 communities to assess project progress as measured by the procurement and installation of materials funded by the intervention.[9]

4.2 Descriptive Statistics

4.2.1 Participant Characteristics

Summary statistics are presented in Table 1. The first 6 columns report means and standard deviations for control villages (Columns 1-2), treatment villages (Columns 3-4) and the full sample (Columns 5-6), while the last column reports p-values for balance between the treatment and control groups across each variable.[10]

Panel A of Table 1 summarizes participant characteristics. Consistent with recruitment protocols, exactly half of the sample in both treatment and control communities is female. Twenty-six percent of participants are under the age of 30, 18% report owning no fixed assets (such as a boat or a bicycle), and only 11% list a primary source of income other than horticulture or fishing. In the analysis, the latter two measures are used to proxy for respondents' income. Fifty-five percent of participants report that they either didn't attend any community meetings over the previous five years or did not speak at any of the meetings. This measure is used to proxy for marginalization in the analysis.[11] Finally, the mean response to the altruism question is 8.3 on a scale from 1 to 10. Across all of the aforementioned characteristics, the sample is balanced between treatment and control groups.

Panel B of Table 1 summarizes participants' *ex-ante* project preferences. More than a third of participants ranked the local kindergarten as their most preferred option, while sanitation (15%) and water supply (12%) were the second and third most popular first choices, respectively (and most frequently ranked as second choices). In 59% of villages, kindergarten was the most frequently reported top-ranked preference among all participants, while sanitation was the most frequently reported top ranked preference in 15% of villages. Participants' preferences over project types are balanced across treatment and control villages, with the exception of those over

[9]Data was not collected from 25 communities due to inaccessibility.

[10]P-values are calculated by regressing each variable on a treatment dummy and province fixed-effects, with standard errors clustered at the village level when the outcome variable is at the individual level, and robust standard errors otherwise.

[11]This measure is significantly negatively correlated with leadership status and wealth and significantly positively correlated with being female and being aged under 30 (Table A1).

Table 1: Summary statistics and covariates balance

	Control		Treatment		Total		P-value
	Mean	**Sd**	**Mean**	**Sd**	**Mean**	**Sd**	
Panel A: Participant Characteristics							
Female	0.500	(0.500)	0.500	(0.500)	0.500	(0.500)	.
Under-30	0.256	(0.437)	0.255	(0.436)	0.256	(0.436)	0.967
No Assets	0.175	(0.380)	0.176	(0.381)	0.176	(0.381)	0.969
Off-Farm Income	0.108	(0.310)	0.121	(0.327)	0.114	(0.318)	0.493
Limited Participation	0.537	(0.499)	0.560	(0.497)	0.549	(0.498)	0.416
Altruism	8.331	(2.324)	8.335	(2.266)	8.333	(2.294)	0.968
Panel B: Primary Project Preference							
Kindergarten	0.357	(0.480)	0.311	(0.463)	0.334	(0.472)	0.256
Primary School	0.102	(0.303)	0.090	(0.286)	0.096	(0.295)	0.518
Health Center	0.064	(0.244)	0.106	(0.308)	0.085	(0.279)	**0.018**
Roads	0.016	(0.127)	0.022	(0.148)	0.019	(0.138)	0.384
Market	0.029	(0.167)	0.045	(0.207)	0.037	(0.189)	0.115
Water	0.121	(0.327)	0.121	(0.327)	0.121	(0.327)	1.000
Sanitation	0.136	(0.343)	0.171	(0.377)	0.154	(0.361)	0.354
Community Bldg.	0.029	(0.167)	0.036	(0.187)	0.032	(0.177)	0.483
Church	0.092	(0.290)	0.076	(0.266)	0.084	(0.278)	0.541

Note: The table reports means and standard deviations (in brackets). P-values from regressions of outcome on treatment and province fixed-effects.

health centers (p = 0.018), while leader preferences are imbalanced over water and sanitation projects.[12] When looking at the aggregate distribution of project preferences across treatment and control villages, a chi-square test does not reject the hypothesis that both overall preferences and leaders preferences are drawn from the same distribution (p = .878 and p = .114, respectively).

Across the sample, leaders preferences diverge from those of other community members, particularly for health, sanitation, and school projects (Figure 1). Specifically, 12.5% of leaders prioritize the improvement of health facilities, compared to 8.5% of participants overall (two-sided t-test, p = .093). Similarly, sanitation is prioritized by 20.6% of leaders versus 15.4% of all participants (p = .038). Conversely, primary schools are preferred by 9.6% of participants generally, but just 5.6% of leaders (p = .026). A chi-square test rejects the hypothesis that the distributions of preferences of these two groups are the same (p = .024).

Among other sub-groups, women have stronger preferences for kindergarten and weaker preferences for sanitation projects than men. Sanitation is also less popular among young (under-30), poor and marginalized participants. Young respondents are also more likely to prioritize primary education projects.[13] Preferences thus appear to align with the types of public goods that individuals are likely to need most (Chattopadhyay and Duflo, 2004).

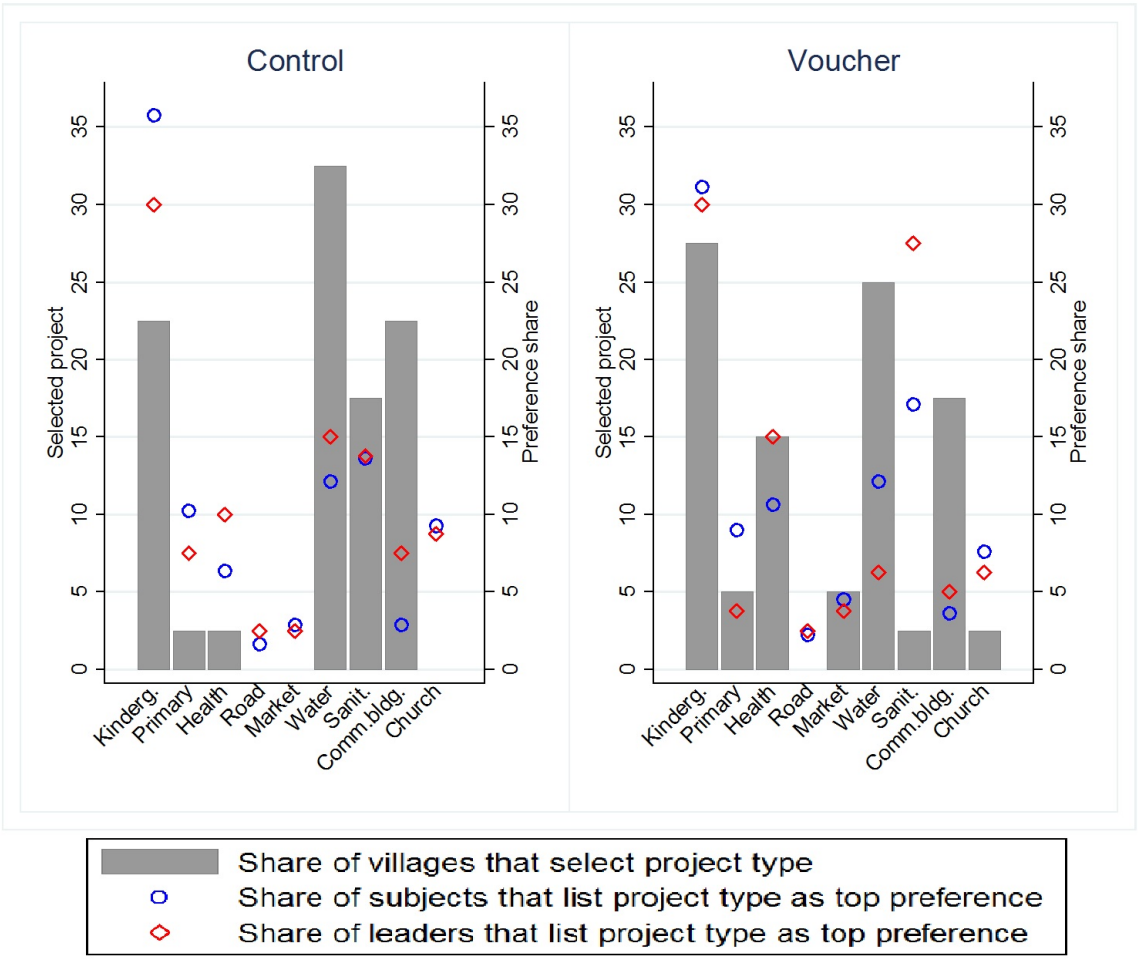
4.2.2 Selection Process

Discussions among participants regarding project selection last an average of 12.7 minutes in control communities and 15.4 minutes in treatment communities, with the difference significant at the five percent level (Table 2). The duration of discussion in the SCA is comparable to that of discussions in similar activities in Casey, Glennerster, and Miguel (2012), which ranged on average between 9 minutes in control communities and 11 minutes in treatment communities.

The effect of the treatment on discussion duration appears to arise from a shifting of the right tail of the distribution: only 15% of control communities have discussions that last 20 minutes or longer, while 38% of treatment communities do so. The increase in the duration of discussions in treatment villages is due to an increase in individual participation, both on the extensive and intensive margins. The treatment increases the share of participants speaking during the discussion by 4.1% points

[12]Table A2 reports balance tests for preferences over projects by other relevant sub-groups.
[13]Results available upon request.

Figure 1: Preferences over project types and project choices



over the control group level of 39.5%. The treatment also causes participants to speak more: the average number of five-minute intervals during which a participant intervenes in the discussion at least once is 0.67 in control villages, compared to 0.83 in treatment villages. These differences are statistically significant at the 5% and 10% levels for duration of the discussion and number of intervals, respectively (Table 2).

Table 2: Voucher treatment increases discussion and improves satisfaction

	Control		**Treatment**		**Total**		**P-value**
	Mean	**Sd**	**Mean**	**Sd**	**Mean**	**Sd**	
Voucher Contributions			29.085	(12.169)			
Speaker	0.395	(0.489)	0.436	(0.496)	0.416	(0.493)	0.216
No. Intervals (a)	0.668	(1.000)	0.835	(1.157)	0.751	(1.083)	0.063
Discussion Duration	12.69	(5.110)	15.38	(6.726)	14.04	(6.087)	0.030
Match All (b)	0.250	(0.439)	0.350	(0.483)	0.300	(0.461)	0.302
Match Leader	0.400	(0.496)	0.375	(0.490)	0.388	(0.490)	0.813
Satisfaction	0.869	(0.338)	0.927	(0.259)	0.898	(0.303)	0.005
Fair Process	0.859	(0.348)	0.912	(0.283)	0.886	(0.318)	0.020

Note: The table reports means and standard deviations (in brackets). P-values from regressions of outcome on treatment and province fixed-effects. "Speaker" is equal to 1 if a subject spoke during the discussion. "No. Intervals" represents the number of five minute intervals during which a subject spoke at least once. "Match All" indicates a correspondence between the selected project and the modal priority of participants.

4.2.3 Voucher Contributions

In communities assigned to the treatment group, contributions by participants averaged SBD 29 (Table 2) and ranged between the feasible minimum of SBD 0 and the feasible maximum of SBD 50, with a mode at SBD 25 (Figure A1). Contributions generally decrease monotonically on both sides of the mode, with the exception that 13.3% of participants contributed the full amount possible. Only 0.6% of participants contributed zero. These contribution levels are higher than those typically observed in laboratory experiments using one-shot games, where contribution rates are often approximately 40%.

Voucher contributions are correlated with individual characteristics (Table 3). Column 1 reports results of the regression of individuals' voucher contributions on

demographic characteristics and province fixed-effects. Leaders contribute about SBD 2.7 more than the average participant. An effect of similar magnitude is observed for participants with off-farm income (that is, of high socioeconomic status). However, not owning any assets is not significantly correlated with lower contributions.[14] Participants under 30 years of age contribute SBD 2 dollars less. Differences in contributions by gender and history of participation are insignificant. Column 2 further indicates that contributions are positively and significantly correlated with participants' altruism (as gauged by survey responses). The coefficients on leader status, age, off-farm income and altruism retain statistical significance in the combined regression (Column 3). These results thereby indicate that more powerful, older, wealthier, and more altruistic individuals contribute larger absolute amounts.

Voucher contributions are also correlated with active participation in the discussion and perceptions of fairness in process and outcomes. Subjects who spoke during the discussion contributed on average SBD 30.1, while those who did not contributed SBD 27.2 (p = .001). Participants who perceive the selection process as fair and are satisfied with the project choice each contribute SBD 2 more than those who do not, although these differences are not statistically significant (p = .385 and p = .106, respectively).

4.2.4 Project Implementation

Three months after the selection process, only a quarter of communities had procured the necessary materials from the hardware suppliers for which the credits were issued. Qualitative evidence garnered by enumerators indicates that most of the communities that had not obtained the materials had been constrained from doing so by remoteness and the infrequency of transport services.

5 Results

The following subsections report results for the tests of the aforementioned hypotheses. In particular, the subsections report estimates of the effect of the treatment on the selection process; on selection outcomes; on perceptions of and satisfaction with the process; and on implementation outcomes. For each set of results, we both estimate the treatment effect and examine heterogeneity in treatment effects.

[14] At the village-level, the share of better-off and poor participants is not significantly correlated with total contribution levels in the SCA.

Table 3: Individual correlates of voucher contributions

	Voucher contribution		
	(1)	**(2)**	**(3)**
Leader	2.607**		2.309*
	(1.224)		(1.269)
Female	-0.547		-0.560
	(0.887)		(0.908)
Young	-2.068*		-2.357**
	(1.091)		(1.089)
Off-Farm Income	2.514*		2.350*
	(1.336)		(1.392)
No Assets	-0.970		-1.567
	(1.383)		(1.367)
Marginalized	-1.073		-0.967
	(1.043)		(1.028)
Altruism		0.580***	0.549**
		(0.204)	(0.205)
DV Mean (a)	29.085	29.172	29.172
DV SD	12.169	12.229	12.229
N	787	755	755
Adj. R-squared	0.023	0.016	0.033

Note: Linear regressions. All regressions include province fixed-effects. Standard errors clustered at the village level in parentheses. (a) DV: dependent variable.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

To account for multiple comparisons that arise because we test effects on multiple outcomes and across multiple subgroups, we follow Anderson (2008). Specifically, we create weighted indices when analyzing multiple outcomes for a single hypothesis (generally those presented in a single table) and sharpened q-values when analyzing multiple subgroups.

5.1 Effects on Selection Process

5.1.1 Treatment Effect

In order to estimate the effect of the treatment on the selection process, we regress participation in the discussion on a dummy equal to 1 if the village was assigned to the voucher treatment and include province fixed-effects (Table 4). The dependent variable is the probability of speaking (Column 1), the number of five minute intervals during which a participant spoke (Column 3),[15] and a weighted index of these two outcome variables (Column 5).

Columns 1, 3 and 5 of Table 4 report the results. Column 5 indicates that, per the aggregate outcome measure, the treatment has a positive effect on participation and that the effect is statistically significant at the 10% level. Vouchers thus generally increase participation in the selection process by 0.13 standard deviation units (the minimum effect detectable in our sample at the 10% level is approximately 0.12 SD units; at the 5% level it is 0.15 SD units). While it is feasible that this increase may arise as a result of coordination among participants on contribution strategies, facilitators reported that such discussions happened in only a few villages. The relatively wide distribution of contributions within villages also suggest that, if any such coordination took place, it was not particularly effective. Specifically, the difference between the smallest and the largest contributions is greater than SBD 30 in 60% of the villages.

5.1.2 Effect Heterogeneity

In order to estimate how the treatment affects different types of participants, we add controls for individual characteristics to the regression model and interact them with the treatment. We consider the following participant characteristics as potential

[15] Given that the probability of speaking in the meeting was affected by the treatment, the analysis of speaking intervals does not censor this outcome at participation equals one, but always considers the unconditional outcome. We thank an anonymous referee for the suggestion.

Table 4: Voucher treatment increases frequency of speaking in SCA discussions

	Probability of speaking		**No. intervals spoken**		**Weighted index**	
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
Treatment	0.0413	0.0461	0.162*	0.233*	0.130*	0.175*
	(0.0331)	(0.0430)	(0.0858)	(0.126)	(0.0728)	(0.0962)
Leader		0.337***		0.699***		0.710***
		(0.0471)		(0.0968)		(0.0842)
Female		-0.201***		-0.466***		-0.446***
		(0.0280)		(0.0664)		(0.0525)
Young		-0.207***		-0.383***		-0.409***
		(0.0340)		(0.0779)		(0.0701)
Off-Farm Income		0.114**		0.203**		0.220**
		(0.0519)		(0.0994)		(0.0985)
No Assets		-0.0281		-0.112		-0.0853
		(0.0398)		(0.0769)		(0.0752)
Treatment x Leader		-0.0196		0.147		0.0392
		(0.0624)		(0.147)		(0.122)
		[1.00]		[1.00]		[1.00]
Treatment x Female		0.00120		-0.136		-0.0703
		(0.0396)		(0.107)		(0.0831)
		[1.00]		[1.00]		[1.00]
Treatment x Young		0.0160		-0.0837		-0.0333
		(0.0531)		(0.105)		(0.102)
		[1.00]		[1.00]		[1.00]
Treatment x Off-Farm		-0.0308		-0.0743		-0.0699
		(0.0740)		(0.177)		(0.157)
		[1.00]		[1.00]		[1.00]
Treatment x No Assets		-0.0320		0.0478		-0.0109
		(0.0571)		(0.140)		(0.123)
		[1.00]		[1.00]		[1.00]
DV Mean Control Group (a)	0.395	0.395	0.668	0.668	-0.066	-0.066
DV SD Control Group	0.489	0.489	0.999	0.999	0.960	0.960
N	1600	1600	1548	1548	1548	1548
Adj. R-sq	0.032	0.161	0.055	0.209	0.041	0.201

Note: Linear regressions. All regressions include province fixed-effects. Standard errors clustered at the village level in parentheses, FDR-adjusted q-values in brackets. "No. intervals spoken" represents the number of five minute intervals during which a subject spoke at least once. (a) DV: dependent variable.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

sources of heterogeneity in treatment effects: leader; female; young (under 30); off-farm income source; and no assets. As in all regressions with multiple interaction terms in the analysis, we offset the increased potential for false positives due to multiple comparisons by adjusting the p-values for the false discovery rate (Anderson, 2008) and reporting sharpened q-values.

Columns 2, 4, and 6 of Table 4 report regression results. While the results show that leaders and subjects with off-farm income are more likely to speak and women and young persons are less likely to do so, there is no evidence that the treatment changes the nature of participation by any of the respective sub-groups.[16]

Of particular interest is the effect of the treatment on marginalized individuals who had not actively participated in previous community meetings (see Section 4.2). While such participants were 32 percentage points less likely than non-marginalized participants to speak during the discussion in control villages (p = .000), the treatment significantly increases their involvement in the discussion. Figure 2 shows the effect of the treatment on the probability of speaking (left) and the number of intervals (right). While the general effect on both outcomes is positive overall, the effect for marginalized participants is larger and attains a higher level of statistical significance (in two-sided t-tests, p = .001 and p = .0001 for the probability of speaking and number of intervals, respectively).[17]

5.2 Effects on Selection Outcomes

5.2.1 Treatment Effect

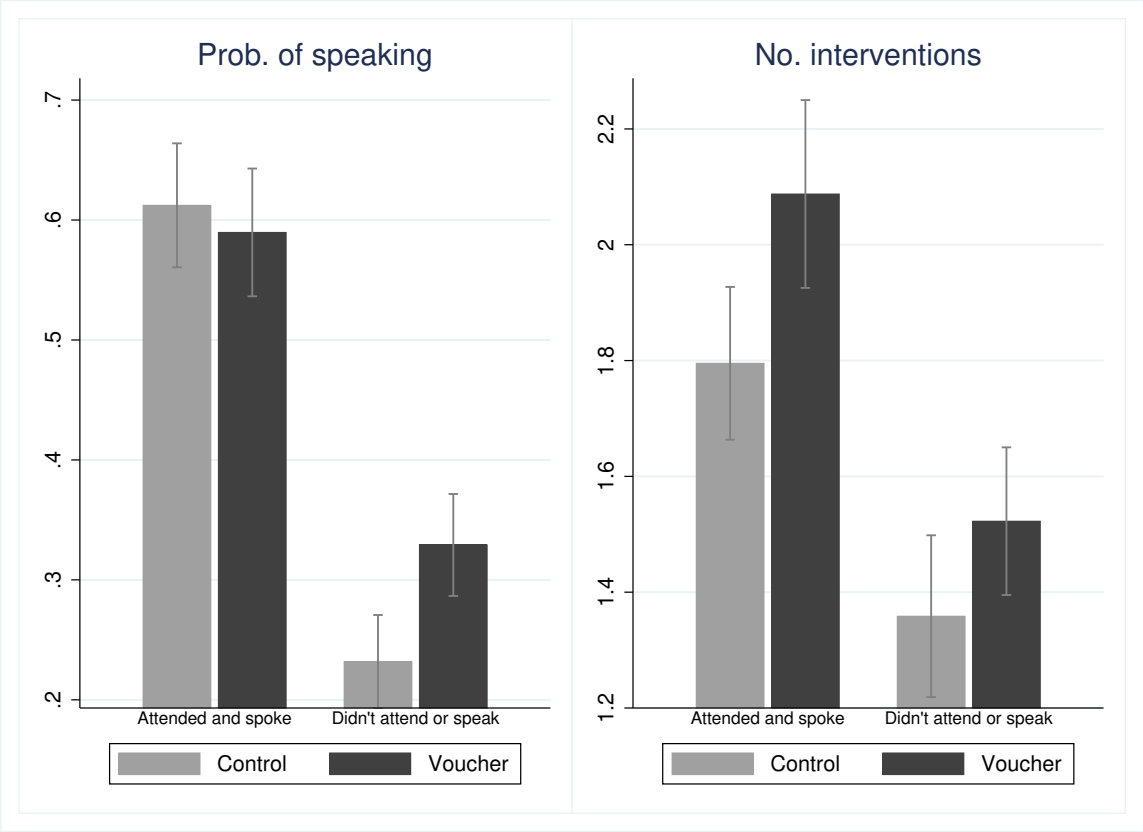
Figure 1 compares selection outcomes in the treatment and control villages with the *ex-ante* primary preferences of participant villagers and participant leaders. Sanitation projects were selected less frequently in treatment (2.5%) than control communities (17.5%; p = .025), while health projects were selected more frequently (15% vs. 2.5%; p = .049).

To identify the overall effect of the treatment on the pattern of selection outcomes, we follow Chattopadhyay and Duflo (2004) in estimating a series of community-level

[16]These results are robust to controlling for imbalanced preferences over project types overall and by leaders, women, and younger participants.

[17]As shown in Table A3, the results on the probability of marginalized individuals speaking retain statistical significance at the 10% level when adding controls for individual characteristics, their interaction with treatment, and imbalanced preferences over project types. The difference in impacts on the weighted index is also large (0.19 SD) but loses statistical significance after adjusting for the multiple subgroup testing.

Figure 2: Voucher treatment increases speaking in discussion, particularly among those who had not done so in recent prior meetings



seemingly unrelated regressions (SUR). Namely, we regress indicators of whether a certain project type was selected on treatment and province fixed-effects. As there is an imbalance between treatment and control groups in baseline preferences over projects,[18] we include controls for all imbalanced baseline preferences in the model.

Table 5 reports the results of the estimation: Column 1 reports the regression coefficients of the treatment indicator, and Column 2 the mean of the dependent variable for control group villages. The results confirm that the treatment increased the probability that health projects were selected and reduced the probability of sanitation projects being selected, with both effects statistically significant at the 5% level. A Wald test of joint significance of the treatment coefficients shows that vouchers significantly affected selection outcomes (p = .073). The minimum effect detectable in our sample at the 5% level varies by project type, from roughly 4.7 percentage points for church buildings to 19.7 percentage points for water systems.

In order to determine whether the treatment resulted in the selection of projects that were more aligned with the preferences of leaders, general participants, or those who were *ex-ante* marginalized, we regress, on the treatment, a binary variable that indicates whether the selected project corresponded with the modal priority preferences of the respective group. In the event of a tie in preferences, correspondence is checked with either of the respective preferences: for instance, in the case of leaders, a project is defined as matching leaders' preferences if it was either the male or the female leader's top-ranked project type.[19] Regressions of this variable are run at the community-level on a treatment indicator with province fixed-effects and controls for imbalanced preferences over project types overall and by leaders and marginalized individuals.

Table 6 reports the results of the estimation, which indicates that the treatment has no statistically significant effect on the probability of the selected project corresponding with the preferences of general participants, leaders, or participants who were *ex-ante* marginalized, or on a weighted index of all three.[20] The minimum effect on the weighted index detectable at the 5% level in our sample is 0.47 SD units. This

[18]Specifically, health projects were more preferred in the treatment group, overall and by women and young people, and sanitation projects were more strongly preferred by leaders in the treatment group

[19]Out of 31 instances of selected projects matching leaders' preferences, seven correspond to cases in which male and female leaders preferences are aligned. Of the remaining instances, male leaders' preferences are matched by project choice in 16 cases, and female leaders' in eight cases.

[20]No statistically significant effects of treatment are observed if we examine agreement with male and female leaders separately. Results available upon request.

Table 5: Voucher treatment increases selection of health centers for improvement in SCA and reduces selection of sanitation improvements

	Treatment coefficient (se)	**Control group mean (sd)**
	(1)	(2)
Dependent variable: selected project is		
Kindergarten	0.050	0.225
	(0.094)	(0.418)
Primary School	0.025	0.025
	(0.041)	(0.156)
Health center	0.126**	0.025
	(0.060)	(0.156)
Market building	0.050	0
	(0.034)	
Water system	-0.073	0.325
	(0.098)	(0.469)
Sanitation	-0.145**	0.175
	(0.063)	(0.380)
Community building	-0.050	0.225
	(0.086)	(0.418)
Church building	0.025	0
	(0.024)	
N	80	

Note: SUR estimation. All regressions include province fixed-effects and imbalanced baseline preferences over project types. Column 1: robust standard errors in parentheses. Column 2: standard deviations in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

constrains what we can conclude from these cross-village average impact estimates on the correspondence of preferences and project choices. However, we do find that more precise impact estimates are possible within sets of villages with varying levels of initial agreement and leader altruism, as discussed below.

Table 6: Voucher treatment does not significantly affect probability that selected project will match participants' preferences

	Match between project and preferences			**Weighted index**
	All	*Leader*	*Marginalized*	
	(1)	**(2)**	**(3)**	**(4)**
Treatment	0.0805	0.0594	0.130	0.0745
	(0.103)	(0.109)	(0.106)	(0.236)
DV Mean Control Group (a)	0.25	0.4	0.25	-0.132
DV SD Control Group	0.439	0.496	0.438	1.052
N	80	80	80	80
Adj. R-sq	0.139	0.159	0.110	0.048

Note: Linear regressions. All regressions include province fixed-effects and imbalanced baseline preferences over project types. Robust standard errors in parentheses.
(a) DV: dependent variable. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

5.2.2 Effect Heterogeneity

Differences in leader and villager preferences may arise as a result of differences in interests or differences in information on the relative benefits of projects (Kosfeld and Rustagi, 2015). Where information asymmetries cause differences in preferences, the resolution of the asymmetry in the course of the discussion may cause villagers to adopt leaders' preferences, resulting in minimal elite capture despite the appearance of such. To isolate the effect of the treatment on 'interest-driven' elite capture (as opposed to more benign forms of 'information-driven' elite capture), we build on the regression models presented in Table 6 by interacting the treatment with measures of the altruism of participating leaders.[21] The corresponding estimates are reported in

[21]The approach assumes that the selection discussion enables villagers to discover whether leaders' preferences are driven by information or interest and that, in the case of the former, that villagers will accede to the leaders' preferences, weakening the treatment effect.

Table A4 and indicate that the treatment differentially improves the correspondence between participants' preferences and selection outcomes where the leader is less altruistic. Results are not statistically significant at conventional levels when we estimate the interaction effects on the correspondence between the selected project and leader preferences, marginalized villager preferences, and a weighted index of the three outcome variables, respectively.

In order to identify how the effects of the treatment are conditioned by the correspondence of preferences of leaders and marginalized participants, we also interact the treatment with a binary measure indicating disagreement between the preferences of these two groups. Results reported in Table A4 indicate that, while neither the treatment nor the interaction have a statistically significant effect on the correspondence between selection outcomes and group preferences when entered individually, the interacted treatment effect is statistically significant at the 10% level when correspondences with the different populations' preferences are combined into a weighted index.[22]

As the treatment may be conditioned by social capital, we also explore interactions between the treatment and community characteristics that may affect cooperation between villagers, including the size of the community, number of tribal factions, nature of local economic activity,[23] distance to the provincial center, and average level of altruism. Table A5 shows that the treatment induces a statistically significant increase in the probability of correspondence between selection outcomes and leader preferences in large communities, and a reduction in such correspondence in remote communities. These effects are, however, at least partially offset by the statistically significant un-interacted effects of village size and distance on the probability of correspondence.

Though the analysis reported in this sub-section is highly explanatory and should be interpreted cautiously, it suggests that the voucher treatment may have differential effects on selection outcomes depending on village characteristics.

[22]The minimum difference in effects across these preference disagreement types that is detectable in our data at the 5% confidence level is 0.97. Our estimated difference is 0.91.

[23]Off-farm income is positively and significantly correlated with voucher contributions and positively correlated with asset ownership.

5.3 Effects on Perceptions of and Satisfaction with Process

5.3.1 Treatment Effect

In order to estimate the effect of the treatment on participants' perceptions of and satisfaction with the selection process, we regress a binary measure of whether a participant perceived the selection process as fair or was satisfied with the selection outcome[24] on a treatment indicator. Regressions control for province fixed-effects and individual characteristics: leadership status, marginalized status, being a woman, being young, earning off-farm income and owning no assets.

Table 7 reports the respective estimates. Perceived fairness (86% in control villages) and satisfaction (87% in control villages) are generally high (see Table 2). The treatment increases perceived fairness by 5.5 percentage points and satisfaction by 6.0 percentage points, with these effects significant at the 5% and 1% level, respectively.[25] The effect on the weighted index is estimated to be 0.20 SD units, a substantively large effect (the minimum detectable effect in our sample is 0.14 SD units). The results also indicate that leaders are more likely to perceive the process as fair, while marginalized individuals are less likely to perceive the process as fair and are less likely to be satisfied with the outcome.

5.3.2 Effect Heterogeneity

In addition to the overall treatment effect, we are also interested in whether the treatment affects the perceptions and satisfaction of leaders and marginalized participants differently than other types of participants. Accordingly, we interact the treatment indicator with those for leaders and marginalized individuals.

The results are reported in Columns 2 and 4 of Table 7. While the treatment does not significantly affect leaders perceptions of or satisfaction with the process, it significantly increases both outcomes for marginalized individuals. All these findings

[24]Data is provided by participants' responses to the questions: *"Do you think the project was chosen in an equitable and fair way?"* and *"Are you personally satisfied with the project that was selected today?"*.

[25]While we cannot completely discount the possibility that these effects are driven by the direct effect of granting vouchers to participants rather than through the indirect effect of improving the quality of the selection process *per se*, there exists a positive and statistically significant correlation between participation in the discussion and both perceptions and satisfaction. In addition, participants that spoke during the discussion made higher voucher contributions, on average. There thus exists *prima facie* evidence to indicate that the improvement in perceptions and satisfaction is driven by the increase in the quality of discussion rather than the mere provision of vouchers.

Table 7: Voucher treatment improves perceptions of fairness and satisfaction with outcomes

	Process perceived as fair		**Satisfied with outcome**		**Weighted index**	
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
Treatment	0.0549**	-0.0138	0.0601***	0.0122	0.204***	-0.00179
	(0.0227)	(0.0292)	(0.0202)	(0.0235)	(0.0706)	(0.0833)
Leader	0.0582***	0.0357	0.0191	0.0224	0.135*	0.102
	(0.0215)	(0.0323)	(0.0217)	(0.0305)	(0.0715)	(0.108)
Marginalized	-0.0486**	-0.107***	-0.0624***	-0.106***	-0.197***	-0.377***
	(0.0228)	(0.0289)	(0.0204)	(0.0272)	(0.0707)	(0.0874)
Treat x Leader		0.0466		-0.00571		0.0701
		(0.0420)		(0.0425)		(0.141)
		[0.157]		[0.808]		[0.452]
Treat x Marginalized		0.117***		0.0884***		0.362***
		(0.0386)		(0.0328)		(0.115)
		[0.007]		[0.018]		[0.005]
DV Mean Control Group (a)	0.859	0.859	0.869	0.869	-0.099	-0.099
DV SD Control Group	0.348	0.348	0.338	0.338	1.107	1.107
N	1600	1600	1600	1600	1600	1600
Adj. R-sq	0.053	0.059	0.054	0.058	0.064	0.070

Note: Linear regressions. All regressions include controls for being female, being young, having no assets, having off farm income, and province fixed-effects. Standard errors clustered at the village level in parentheses.
(a) DV: dependent variable. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

hold for the weighted index of these outcomes (Columns 5 and 6).[26]

5.4 Effects on Project Implementation

5.4.1 Treatment Effect

Notwithstanding the constrained sample and the lack of variation in the outcome indicator relating to project implementation (Section 4.2.4), we estimate the effect of the treatment on project implementation by regressing an indicator (equal to 1 if a community picked up the construction materials in the three months following the meeting) on treatment and province fixed-effects. As shown in Table 8, we find no statistically significant effects of treatment on implementation.

It is interesting to note that there is no statistically significant correlation between contributions in the SCA and project implementation (p = .45). Since we also do not find any significant correlation between contributions and prior issues in implementation with RDP projects, as reported in our village survey, it appears that features other than implementation success were more salient in participants' decision-making around contributions.

5.4.2 Effect Heterogeneity

We also examine effect heterogeneity by interacting the treatment with a binary variable indicating whether the community is located farther than the median travel time from the respective provincial center. As with the basic regression, we find no evidence to indicate that the treatment affected the probability of the participants retrieving materials to complete the project, regardless of whether the community was closer to or farther away from the respective provincial center.

6 Discussion

While vouchers significantly change the pattern of projects that are selected, they do not reduce the probability of correspondence between the selected project and the *ex-ante* preferences of leaders and do not increase the probability of correspondence between the selected project and the *ex-ante* preferences of marginalized and/or

[26]We further test the robustness of these results by controlling for imbalanced preferences over projects and separately interacting the treatment with individual characteristics, and find qualitatively consistent results.

Table 8: Voucher treatment does not affect project implementation

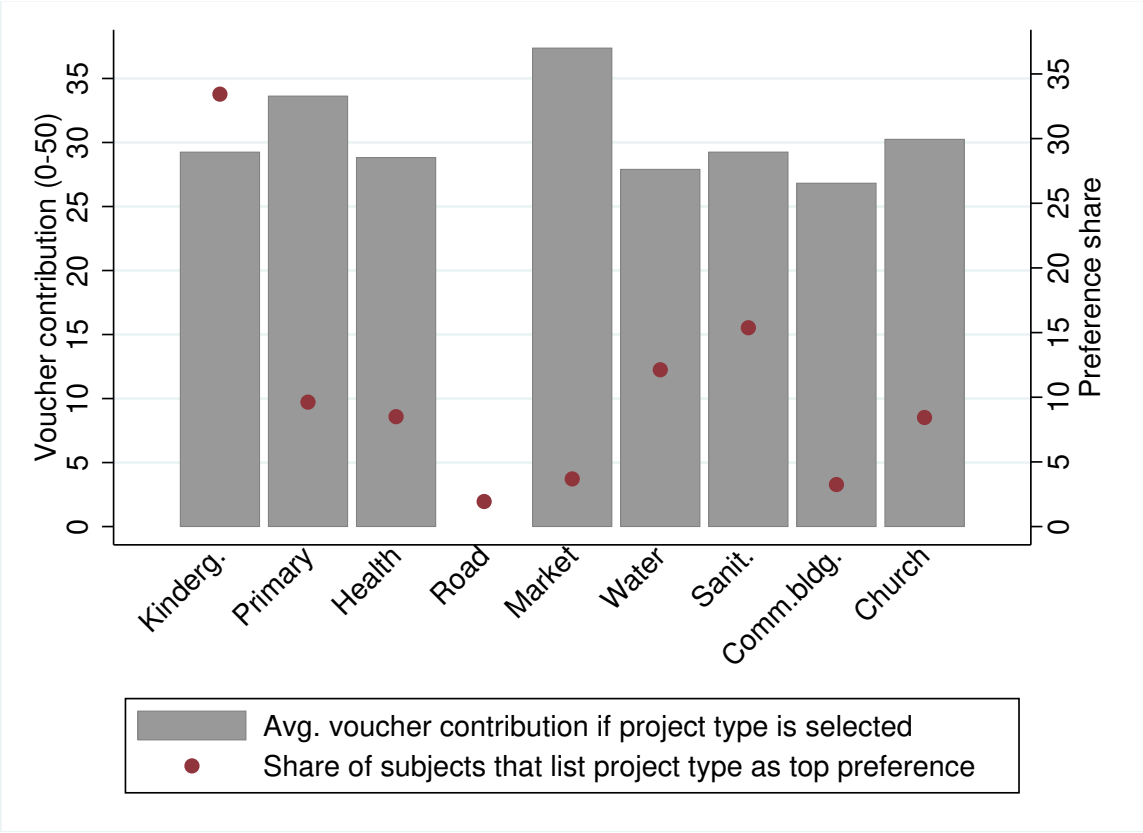
	Picked up materials	
	(1)	(2)
Treatment	-0.0122	0.262
	(0.111)	(0.181)
Far from province center		0.241
		(0.170)
Treatment x Far		-0.290
		(0.246)
DV Mean Control Group (a)	0.294	0.258
DV SD Control Group	0.462	0.445
N	65	58
Adj. R-sq	0.031	0.058

Note: Linear regressions. All regressions include province fixed-effects. Robust standard errors in parentheses. (a) DV: dependent variable. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

non-leader participants. *Prima facie*, such results suggest that vouchers have no impact on elite capture. However, vouchers do increase the duration of discussions and the number of participants that contribute to discussions, and it is feasible that, with these improvements in the quality of discussions, participants may update their preferences. Accordingly, the correspondence between the selected project and participants' *ex-ante* preferences may not be informative as a true measure of elite capture. In order to better understand whether the change in the pattern of selected projects induced by vouchers is likely to be beneficial to non-leader participants, we further examine the absolute and relative characteristics of projects selected in treatment communities.

First, we examine the relationship between selected and *ex-ante* preferred projects and voucher contributions in treatment communities (Figure 3). If participants' preferences are not significantly affected by the discussion, we would expect participants to contribute more where the selected project corresponds with their *ex-ante* preferred project. However, when we compare voucher contributions and baseline project preferences, we observe little correlation between the two distributions. This visual impression is confirmed when we regress participants' voucher contributions

Figure 3: Participants do not contribute more when their preferred project is selected



Note: Grey bars report the avg. contribution by project type selected in the SCA; red dots indicate the avg. share of subjects who at baseline list each project type as their top preference.

on a binary variable that denotes the correspondence between selected projects and the same participant's top-ranked project, controlling for individual characteristics, imbalanced baseline preferences over project types, and province fixed-effects. We find that participants do not contribute significantly more when their *ex-ante* preferred project is selected.[27] Accordingly, there is indicative evidence that discussions alter participants' preferences.

Second, we examine whether the treatment led to the selection of projects that are in greater need of funding, as proxied by survey data on community members perceptions of improvements in the quality of different public facilities over the previous five years. Specifically, we run village-level SURs of the probability that a specific project type is selected on the interaction between treatment and an indica-

[27]The regression coefficient indicates that contributions are 1.997 SBD higher on average when there is a match (s.e. = 1.306; p = .134). Results available upon request.

tor of perceived past improvements, controlling for baseline project preferences and province fixed-effects. We find no statistically significant effect of the treatment on the likelihood that previously un-improved projects were selected (p = .239).

Finally, we further examine whether the treatment led to the selection of projects which complemented on-going projects funded by RDP. The assumption is that the limited funding provided through the experiment could be put to better use for incremental improvements or maintenance activities of existing public goods, rather than for constructing new ones. We run village-level SURs of the likelihood that a specific project type is selected on the interaction between the treatment and an indicator of RDP-selected project type, controlling for baseline project preferences and province fixed-effects. Again, we do not find any statistically significant effect of the treatment on the likelihood that the experiment and RDP-selected project types match (p = .210). However, there exists a correlation between the distribution of projects chosen during the SCA and RDP projects. Specifically, a chi-square test cannot reject the hypothesis that projects selected by the experiment and RDP are drawn from the same distribution in treated villages ($\chi^2 = 91.8$; p = .000), but does reject the hypothesis for control communities ($\chi^2 = 34.981$; p = .770).[28] Figure 4 shows the frequency of projects selected in the SCA and by RDP in treated and control villages and confirms the closer correspondence between the two variables in the former set of villages.

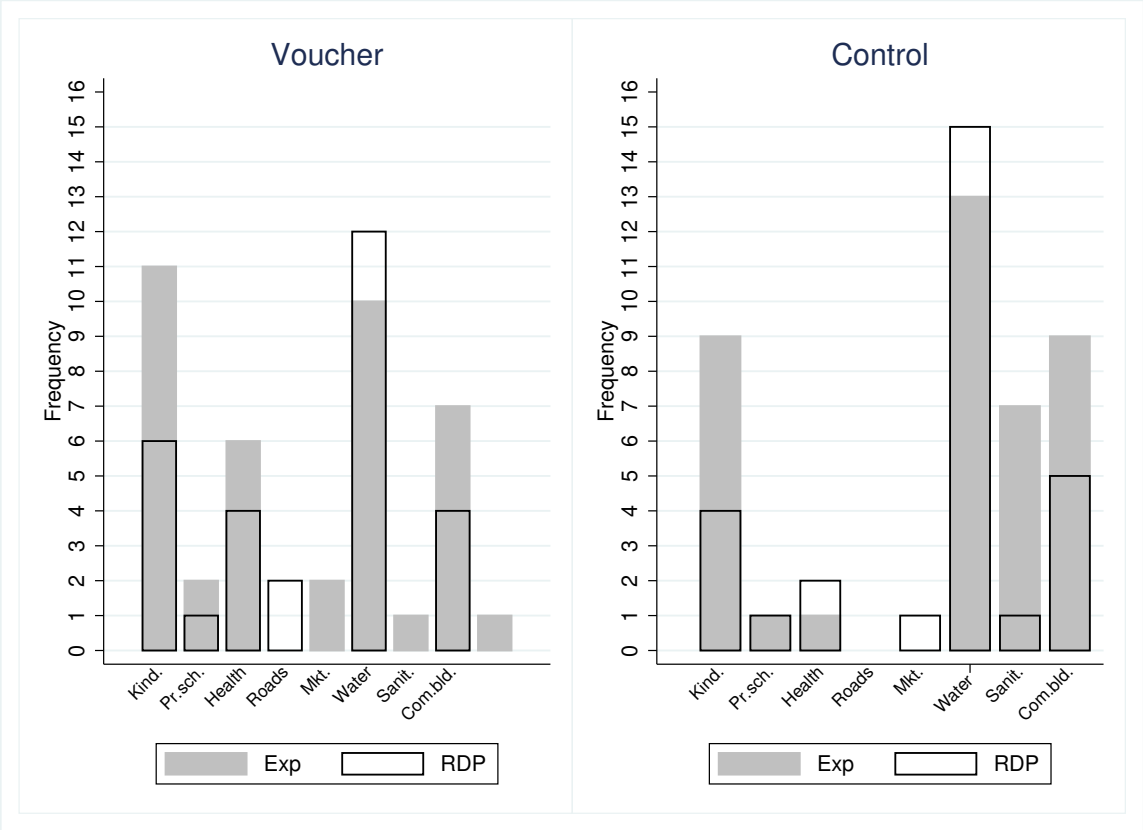
Overall, these results suggest that the increase in the quality of discussion induced by the voucher-based modality changes participant preferences, potentially as participants inform one another about the ongoing RDP project and the efficiency gains from making complementary investments. These potential mechanisms suggest that the vouchers may promote a more rational allocation of resources to improve existing public goods.

7 Conclusions

In order to both improve the correspondence between development projects and local preferences and to encourage higher citizen participation in project implementation, development programming has become increasingly decentralized over recent decades. However, the equity and efficiency of such programs has been hampered by limited participation among community members and by the propensity of local elites

[28]The distribution of RDP-selected projects does not differ between treated and control villages.

Figure 4: Voucher treatment does not affect whether SCA projects match RDP projects



to exercise undue influence over project selection. While various initiatives, such as community fora, referenda, and community participatory monitoring, have been devised to constrain elite capture and increase community involvement, these have experienced limited success. This paper presents an alternative scheme, which seeks to reduce elite capture and increase community engagement by providing vouchers to community members participating in a local resource allocation exercise.

Estimates from the experiment indicate that vouchers increase the duration of the discussion and the probability of otherwise-marginalized individuals participating in the discussion. There is also evidence that vouchers change the type of projects selected and, while there is no general evidence that vouchers alter the probability of selected projects corresponding with participant preferences, there is some evidence that vouchers increase the probability of marginalized individuals realizing their preferences when those preferences disagree with those of leaders. In addition, vouchers improve the perceived fairness of and satisfaction with the selection process among both participants generally, and marginalized participants specifically.

The available data does not provide any indication that vouchers affect project implementation outcomes in the three months after the treatment, potentially because of the limited progress in implementation observed across the sample. Nonetheless, the vouchers' effects on participant involvement and community satisfaction with the project selection process suggest that, over a longer time, vouchers may improve community member engagement in project monitoring and thus the quality of implemented projects. However, as vouchers also generally reduce the funding that is provided to public works projects (because some funding is kept by individual participants), the vouchers may constrain the scope of funded projects and thus provide more limited benefits. Exploring the longer-term effects of vouchers on project outcomes is an important avenue for subsequent research.

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Appendix

Figure A1: Distribution of voucher contributions

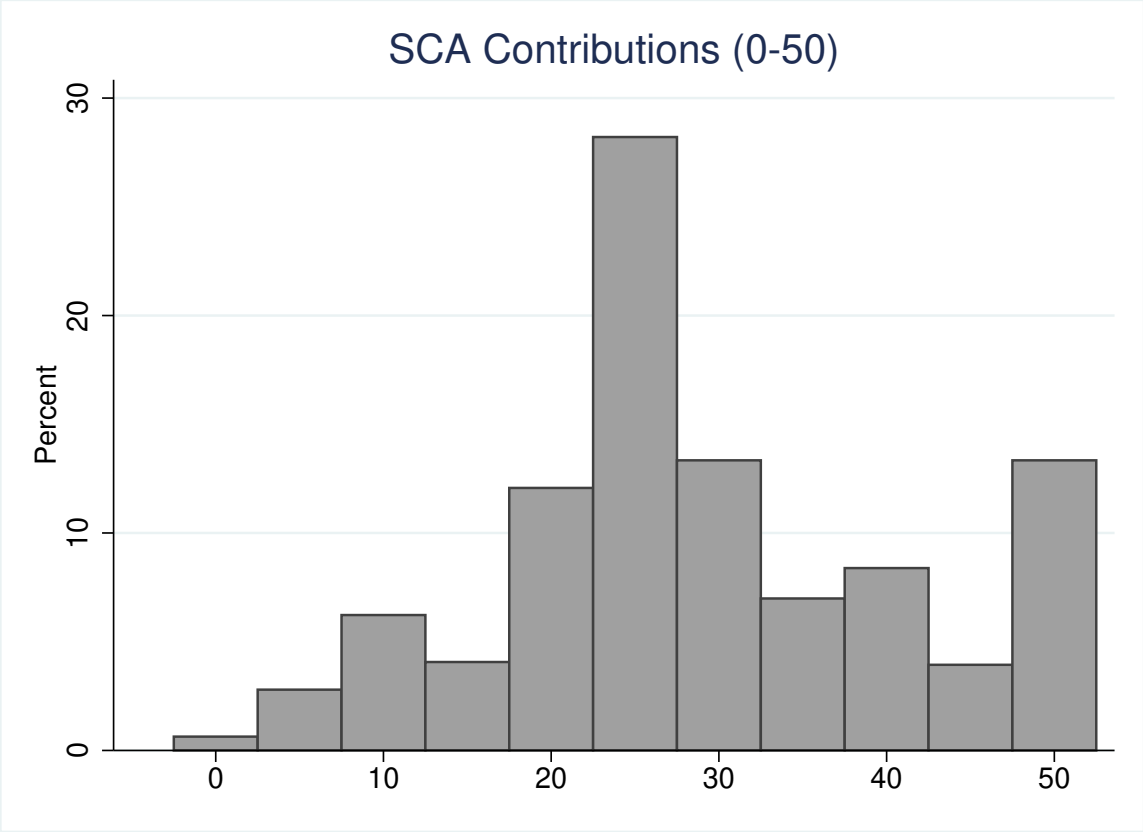

Table A1: Correlates of individual participation in previous community meetings

	Attended and Spoke **(1)**
Leader	0.263***
	(0.036)
Female	-0.261***
	(0.027)
Young	-0.125***
	(0.028)
No assets	-0.035
	(0.034)
Off farm	0.085**
	(0.037)
DV Mean (a)	0.451
DV SD	0.498
N	1600
Adj. R-sq	0.141

Note: Linear regression. Includes province fixed-effects.
Standard errors clustered at the village level in parentheses.
(a) DV: dependent variable. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table A2: Summary statistics and balance of preferences over project types by sub-group

	Kindy (1)	**Pr.sch. (2)**	**Health (3)**	**Roads (4)**	**Mkt. (5)**	**Water (6)**	**Sanit. (7)**	**Comm. (8)**	**Church (9)**
All	0.256	0.518	**0.018**	0.384	0.115	1.000	0.354	0.483	0.541
Leader	1.000	0.333	0.356	1.000	0.631	**0.075**	**0.024**	0.507	0.567
Female	0.297	0.844	**0.085**	0.649	**0.041**	0.879	0.353	0.851	0.154
Young	0.361	0.296	**0.086**	**0.041**	0.891	0.564	0.958	0.978	0.401
No asset	0.689	0.181	0.135	0.604	0.190	0.955	0.858	0.218	0.983
Off farm	0.401	0.465	0.600	0.888	0.217	0.685	0.996	0.470	0.276
Marg. (a)	0.241	0.987	**0.027**	0.174	0.542	0.993	0.527	0.439	0.973

Note: p-values from regressions of outcome on treatment and province fixed-effects. Standard errors clustered at the village level. (a) didn't speak or attend prior community meetings.

Table A3: Treatment increases marginalized participants' speaking in discussion

	Speaker (1)	**No. Interv. (2)**	**Index (3)**
Treatment	-0.00964	0.182	0.0976
	(0.0473)	(0.149)	(0.112)
Marginalized	-0.287***	-0.526***	-0.556***
	(0.0436)	(0.0903)	(0.0859)
Treat x Marginalized	0.124**	0.132	0.191*
	(0.0533)	(0.121)	(0.107)
	[0.097]	[0.871]	[0.525]
DV Mean Control Group (a)	0.395	0.669	-0.066
DV SD Control Group	0.489	0.999	0.960
N	1600	1548	1548
Adj. R-sq	0.201	0.244	0.242

Note: Linear regressions. All regressions include individual controls (leader, female, young, no assets, off-farm income), their interaction with treatment, imbalanced baseline preferences over projects and province fixed-effects. Standard errors clustered at the village level in parentheses. (a) DV: dependent variable. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table A4: Treatment improves match of preferences and projects where leaders and participants disagree

	Match between project and preferences						**Weighted index**	
	All		**Leader**		**Marginalized**			
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**
Treat.	0.850	-0.251	0.126	0.0848	0.588	-0.0833	2.132	-0.423
	(0.631)	(0.202)	(0.702)	(0.222)	(0.676)	(0.213)	(1.489)	(0.474)
Altruistic leader	0.0463		-0.0149		0.0379		0.157	
	(0.0445)		(0.0487)		(0.0470)		(0.106)	
Treat x Altruistic	-0.122*		-0.0110		-0.0725		-0.265	
	(0.0724)		(0.0802)		(0.0777)		(0.172)	
Disagreement		-0.116		-0.150		-0.193		-0.325
		(0.140)		(0.165)		(0.148)		(0.330)
Treat x Disagreement		0.263		-0.0120		0.201		0.914*
		(0.208)		(0.232)		(0.219)		(0.486)
DV Mean Control Group	0.25	0.25	0.4	0.4	0.25	0.25	-0.132	-0.132
DV SD Control Group	0.439	0.439	0.496	0.496	0.439	0.439	1.052	1.052
N	80	80	80	80	80	80	80	80
Adj. R-sq	0.183	0.169	0.119	0.139	0.084	0.094	0.034	0.044

Note: Linear regressions. All regressions include province x treatment fixed-effects and control for imbalanced preferences over project types. Robust standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table A5: Treatment reduces likelihood project choice reflects leader's preferences in remote villages and increases likelihood in larger villages

	Match between project choice and leader preferences					
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
Treatment	-0.05	-0.056	0.035	0.287*	-0.024	-1.027
	(0.117)	(0.197)	(0.172)	(0.169)	(1.035)	(1.064)
Large	-0.395**					-0.449*
	(0.186)					(0.226)
Treat x Large	0.578**					0.944***
	(0.256)					(0.325)
						[0.029]
No. Tribal Groups		-0.011				0.002
		(0.025)				(0.025)
Treatment x Tribal Groups		0.026				-0.04
		(0.034)				(0.038)
						[0.213]
Off-Farm			-0.547			0.733
			(0.970)			(0.986)
Treatment x Off-Farm			0.262			-1.479
			(1.193)			(1.274)
						[0.213]
Remote				0.201		0.379**
				(0.148)		(0.161)
Treatment x Remote				-0.328		-0.492**
				(0.216)		(0.232)
						[0.084]
Altruism					-0.034	-0.211**
					(0.099)	(0.104)
Treatment x altruism					0.01	0.201
					(0.124)	(0.132)
						[0.157]
DV Mean Control Group (a)	0.4	0.385	0.4	0.405	0.4	0.389
DV SD Control Group	0.496	0.493	0.496	0.498	0.496	0.494
N	78	76	80	72	80	70
Adj. R-sq.	0.320	0.277	0.246	0.365	0.243	0.472

Note: Linear regressions. All regressions include province fixed-effects and imbalanced baseline preferences over project types. Robust standard errors in parentheses, FDR-adjusted q-values in brackets. (a) DV: dependent variable. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$